

~~#top.~~
H2-P

SPACE, SOCIETY AND SOCIAL SCIENCE

N 63 18864

Code-1

by John R. Seeley
Bertram M. Gross
Sumner Myers
Lewis A. Dexter
Edward E. Furash

OTS PRICE

XEROX \$ 4.60 ph
MICROFILM \$ 1.46 mf

Report to
The Committee on Space Efforts and Society of
The American Academy of Arts and Sciences.

NASA GRANT: NSG-253-62

NOT FOR PUBLICATION

CR-50,437

PREFACE

The ideas and recommendations presented in this report are based on the materials and working papers prepared by the resident members of the 1962 Summer Study Group that was sponsored by the Committee on Space Efforts and Society of the American Academy of Arts and Sciences. These materials were edited and arranged in this report by Edward E. Furash of Harvard Business School, using an earlier version by Lewis A. Dexter. Chapters Two and Three of this report draw chiefly on the work of John R. Seeley, and Chapter Four is based mainly on the work of Mr Seeley and Lewis A. Dexter. Chapter Five draws on the work of all the resident members and of William Harris of the Battelle Memorial Institute. The materials of Appendix A and Appendix B (available separately) are clearly attributed to each author. While the substance and conceptions of this report were approved by the members of the Summer Study Group, the specific phrasing of this printed version is the responsibility of the editors.

We wish to express our appreciation to Mrs. Shirley Nourse Garand and Miss Joan C. Laws for their genuinely valuable patience and assistance during our summer meetings, and for their many hours of work in preparing this document.

John R. Seeley

Bertram M. Gross

Sumner Myers

Lewis A. Dexter

Edward E. Furash

Cambridge, Massachusetts
February 1963

HIGHLIGHTS AND TABLE OF CONTENTS

	Page
<u>CHAPTER ONE: INTRODUCTION</u>	
I. HISTORY OF THE PROJECT: A brief history of the Committee on Space Efforts and Society and the 1962 Summer Study Group	6
II. THE REPORT: A summary of this report	8
III. SOME COMMENTS: A preface by the members of the Summer Study Group	9
<u>CHAPTER TWO: THE SPACE PROGRAM AS A SOCIAL PHENOMENON</u>	
I. INTRODUCTION: The space program, like any other technological program, is a social as well as a scientific phenomenon	10
II. THREE CATEGORIES OF SOCIAL PHENOMENA: (A) The Space Program as Massive Technological Innovation; (B) The Space Program as the Focus of a Number of Different Purposes and Interests; and (C) The Space Program as Initiator of Changes in the Organization of Society	11
III. CONCLUSION: Though our categories of social phenomena are meaningful, they are far from a listing of feasible research projects	14
<u>CHAPTER THREE: THE NATURE OF SOCIAL SCIENCE RESEARCH</u>	
I. INTRODUCTION: The major difficulty in designing research projects is the nature of social science research itself	15
II. NOTES ON SOCIAL SCIENCE RESEARCH: Many interesting social problems are not readily researchable by present social science research techniques and methods	15
Good social science research is characterized by the way in which it is carried out and by its being reported in such a fashion that its most general implications can be inferred readily	16
More significantly, there simply are not enough qualified people able and willing to undertake serious social science research in new areas...	17
The closer social science research comes to examining vital questions of policy the more it will be impugned as being "controversial"	18
III. CONCLUSION: The portrait of social science research presented in this chapter emphasizes its diffuse, unboundaried, "experimental" nature; consequently, good research takes time to plan, and the wise sponsor will plan his research program to allow for that time	19

CHAPTER FOUR: THE ROLE OF THE NATIONAL AERONAUTICS
AND SPACE ADMINISTRATION

Page

I. INTRODUCTION: NASA should sponsor social science research in such a manner as to encourage broad, unfettered inquiry into the nature of space efforts as social phenomena.....	21
II. GENERATING SOCIAL SCIENCE INTEREST IN SPACE EFFORTS: Suggestions for ways in which NASA can generate sustained social science interest in studying the space program as a social phenomenon.....	22
III. SPONSORING BROAD SOCIAL SCIENCE RESEARCH ENQUIRIES: Four methods for sponsoring broad, unfettered study of the space program as a social phenomenon: (A) The Need for Unrestricted Social Science Grants; (B) Employing Social Scientists in NASA; (C) Employing Social Scientists in Contractor Operations; (D) Establishing University Chairs and Sponsoring Seminars.....	24
IV. CONCLUSION: No one knows exactly what is an adequate and feasible scale of social science research for a project as large as our space efforts, but our best judgement is that one mill of every operational dollar expended by NASA is an appropriate rate at which to begin.....	26

CHAPTER FIVE: THE ROLE OF THE COMMITTEE ON SPACE EFFORTS
AND SOCIETY

I. INTRODUCTION: The Committee on Space Efforts and Society should plan to operate and sponsor projects for a four to five year period in order to produce sound and significant social science research.....	28
II. NOTES ON DESIGNING RESEARCH ON THE SPACE PROGRAM AS A SOCIAL PHENOMENON: Social Science research on the space program as a social phenomenon requires two activities, search and planning, prior to the actual undertaking of a research program.....	29
Research on the space program as a social phenomenon must investigate the immediate, intermediate and remote impacts and repercussions on our social structure.....	30
Engineers and natural scientists should be encouraged to participate in social science research on the impacts of space efforts.....	31
Other nations and social scientists from other nations should participate in research on social phenomena generated by space efforts.....	31
III. CONCLUSION: A WORK PROPOSAL: We propose a two-pronged research strategy in which the Committee on Space Efforts and Society sponsors systematic research in five major project areas, and reserves some funds for smaller scale, unguided inquiries.....	33
APPENDIX A and APPENDIX B.....	43

CHAPTER ONE:

INTRODUCTION

I. HISTORY OF THE PROJECT

In the Spring of 1962, the American Academy of Arts and Sciences received a grant of \$181,000 from the National Aeronautics and Space Administration (NASA) for the purpose of studying, over a period of approximately two years, the relationships of space efforts to U. S. society. To carry out this study, the Academy established a Committee on Space Efforts and Society:

Raymond A. Bauer, Harvard Business School, Chairman
Francis M. Bator, Massachusetts Institute of Technology
Saville R. Davis, The Christian Science Monitor
Donald B. Marquis, Massachusetts Institute of Technology
Don K. Price, Harvard University
Walter A. Rosenblith, Massachusetts Institute of Technology
Earl P. Stevenson, Arthur D. Little, Inc.
Arthur E. Sutherland, Harvard Law School
Lewis A. Dexter, Executive Director

The NASA grant was phrased in broad terms with the expectation that this Committee would examine, as its first task, the part that social science research could play in studying the relationships of space efforts to society, and that, on the basis of this examination, the Committee would identify specific problems worthy of research. Then, as the second phase of its work, the Committee would support or carry out "detailed studies, as mutually agreed upon by NASA and the grantee, of specific problems identified in phase 1."

To help it examine the role of social science research and identify those problems generated by space efforts worthy of research, the Committee invited a group of scholars and administrators to work with its Executive Director during July and August of 1962. This Summer Study Group consisted of Resident Members who participated for two full months; Visiting Members who participated for extended periods of time (usually ten days); Contributors who made formal presentation to the members and led discussion on the presentation, usually for one day; and Guest Observers and Discussants who joined members periodically to hear contributors and to participate in discussions. Exhibit One presents a list of the Summer Study Group participants.

Exhibit 1

LIST OF PARTICIPANTS

American Academy of Arts and Sciences
Committee on Space Efforts and Society

SUMMER STUDY GROUP
1962

RESIDENT MEMBERS

John R. Seeley, Chairman, Department of Sociology, York University, Toronto;
Associate, Department of Psychiatry, University of Toronto
Bertram M. Gross, Professor of Political Science, Syracuse University; and
Visiting Professor, Harvard Business School, 1962-3
Sumner Myers, Senior Engineer, Institute of Public Administration
Lewis A. Dexter, Executive Director, Committee on Space Efforts and Society

VISITING MEMBERS

James Bright, Professor of Business Administration, Harvard Business School
Abba P. Lerner, Professor of Economics, Michigan State University
Harold Mendelsohn, Professor, School of Communications Arts, University of
Denver
Derek de Solla Price, Chairman, Department of History of Science and Medicine,
Yale University

CONTRIBUTORS

Robert Barre, National Aeronautics and Space Administration
William Harris, Assistant to the Vice President, Washington Office, Battelle
Memorial Institute
Edward Hincks, Director of Research, Aerospace Industries Association of
America, Inc.
John P. Hagen, National Aeronautics and Space Administration
Leonard Lederman, Committee on Science and Technology, United States
Chamber of Commerce
Richard Wright, National Aeronautics and Space Administration

GUEST OBSERVERS AND DISCUSSANTS

Raymond A. Bauer, Professor of Business Administration, Harvard Business
School; and Chairman, Committee on Space Efforts and Society
Edward E. Furash, Instructor in Business Administration, Harvard Business School
Earl P. Stevenson, President, Greater Boston Chamber of Commerce, and
Consultant to Arthur D. Little, Inc.
Norman Storer, Assistant Chairman, Department of Social Relations, Harvard
University
Gerhard Wiebe, Dean, School of Public Relations and Communications, Boston
University

II. THE REPORT

The members of the Summer Study Group decided to interpret their assignment from the Committee on Space Efforts and Society as a request for a report which would:

- A. indicate the ways in which the space program is a social phenomenon;
- B. discuss the degree to which social science research could profitably study the space program as a social phenomenon;
- C. suggest the most appropriate role for such agencies as NASA to take as sponsors or doers of social science research;
- D. suggest the role which the Committee on Space Efforts and Society should take as sponsor or doer of such social science research; and
- E. present the characteristics of significant, meaningful and feasible social science research on the space program as a social phenomenon, together with a list of specific research projects that the Committee on Space Efforts and Society could sponsor or undertake.

This report is the Summer Study Group's response to its assignment.

In Chapter Two, "The Space Program as a Social Phenomenon," we point out that space efforts themselves are a social phenomenon, and that they generate additional social phenomena through their impact on society. We describe and discuss three broad categories of significant and worthwhile social science research on the space program as a social phenomenon: (A) the space program as part of a wave of massive technological innovation; (B) the space program as the focus of a number of different purposes and interests; and (C) the space program as an initiator of changes in the organization of society.

In Chapter Three, "The Nature of Social Science Research," we present a portrait of social science research that emphasizes its diffuse, unboundaried, "experimental" nature. We state that the nature of social science research is such that the tasks of formulating the research problem and devising the research techniques are far from easy and routine. We conclude that "good" social science research is characterized by the way in which it is carried out and by its being reported in such a fashion that its most general implications can be inferred readily; and that the most important element in obtaining "good" research is the ability of the researcher. Consequently, the "experimental" nature of social science research leads us to emphasize that good research takes time to plan, and that the wise sponsor will plan his research program to allow for that time.

In Chapter Four, "The Role of the National Aeronautics and Space Administration," we state that NASA will best achieve its own need for excellent social science research and will, at the same time, best benefit the development of the social sciences in general by encouraging broad, unfettered enquiry through unrestricted, long-term grants. To do this, NASA will have to first generate sustained social science interest in studying the space program as a social phenomenon.

In Chapter Five, "The Role of the Committee on Space Efforts and Society," we apply the conception of "lead time" to the role of the Committee as a sponsor and doer of social science research. Specifically, we suggest that the Committee should plan to operate and sponsor projects for a four to five year period in order to produce sound and significant social science research and to encourage others to undertake research in this area. Consequently, we emphatically recommend that the Committee raise, by application to NASA or elsewhere, the funds necessary for a four to five year program. In the remainder of the chapter we present our suggestions on the standards to be used in designing or evaluating specific research on space efforts as social phenomena, together with a general research strategy and specific research projects that the Committee can undertake itself or can recommend to NASA.

There are two Appendices to this report which have not been included in this printed version. They are available separately in mimeograph form. Appendix A contains those essays by members of the Summer Study Group which discuss in greater detail the five projects proposed in Chapter Five of this report. Appendix B contains general essays by members of the Summer Study Group. A description of these Appendices together with a list of their contents will be found at the end of this report.

III. SOME COMMENTS

The range of possible analyses of social phenomena generated by the space program is limitless. This factor alone determines a major criterion we have used in writing this report: that in order to develop feasible work programs that give a possibility of results in a limited time, priorities must be set and the areas of inquiry limited. However, the setting of these priorities is always somewhat arbitrary, and accordingly, we have largely confined ourselves to recommendations in the fields in which we have some competence: the social sciences including therein political philosophy. We have also, and for the same reason, confined our attention to the effect of the space program as a massive technological effort on this planet. We recognize that an equally convincing set of recommendations could be drawn up on the social and bio-psychological problems of living or colonizing outer space, or on problems posed by disciplines with which we are not familiar, such as human ecology. Other reports might well be prepared on these matters.

One final note. Though we emphasized research in this report, we do not wish to leave the impression that the only contributions that social scientists can make to understanding the space effort and its impact or implications for society is through research, particularly new, empirical research. Persons with specialized training can often contribute sound advice based on the accumulated wisdom of their disciplines. Such advice, given in those areas relevant to the person's specialization, could be as valuable and certainly would be less costly than new research. In addition, we do not wish our emphasis to encourage the proliferation of research for its own sake. If organizations are not prepared to receive or use the research being done, then the research may be wasteful. This does not mean that we wish to discourage complementary efforts on the part of different social scientists. Rather, we are emphasizing that a program of effective research requires more than the mere multiplication of new, empirical researches.

CHAPTER TWO:

THE SPACE PROGRAM AS A SOCIAL PHENOMENON

I. INTRODUCTION

Our national space effort is a massive expenditure of physical resources and human energies organized to achieve the radical and novel technology needed to enable man to explore, survive and function in outer space. The space program, like any other technological program, is a social as well as a scientific phenomenon.

Technological programs are a social phenomenon in that the habits, values, attitudes, traditions and institutions of any society determine before the fact whether scientific phenomena of any kind will ever occur. And these same habits, values, attitudes, traditions and institutions determine whether and how such scientific phenomena develop further, how they are interpreted by the society as a whole, and what effects they will have upon the society. This means, in effect, that technological programs have consequences for a society beyond their direct scientific goals; have impacts which can in turn help determine the habits, values, attitudes, traditions and institutions of the society in the future. Stated so generally, these relationships and impacts appear almost too obvious to mention. Yet when one attempts to trace them specifically, especially to describe the societal consequences of a given technological program, the potentially great number and magnitude of these impacts upon the society becomes clear, and, consequently, the need to study and predict them becomes more apparent and urgent.

Examining our national space program in this light, we find that its vast expenditure of money and manpower and its new research all lead us to recognize that the potential and existing social phenomena generated by our space effort are worthy of urgent social scientific study. Yet, at the same time, the potential impacts upon our society are almost numberless, and the mere task of enumerating and classifying them would be gargantuan. Fortunately, a previous effort in this area has already performed the valuable service of identifying and describing the many major social problems that have been, could be, or will be created or accentuated by our space efforts. This Brookings Institution report, "Proposed Studies on the Implications of Peaceful Space Activities for Human Affairs," was prepared by Donald N. Michael in collaboration with a committee of experts from several fields, and emphasizes the desirability of stimulating research, both inside and outside the government, which can help in dealing with these social problems:

Space activities require great investments of money, men, materials, and creative effort and thereby compete with the needs of other areas of human endeavor. They contribute to rapid rates of technological change and thereby give rise to social and personal readjustment problems. Thus it is most desirable that the problems and opportunities they may imply for society be understood. Since the potentialities of space activities are wide ranging, so, too, must be a research program

on their implications. Examined herein are the problems and opportunities that may be introduced by hardware, such as a weather satellite forecasting system; events, such as the adventures of astronauts in space; and ideas, such as those embodied in discussions of the degree to which national prestige may be dependent on success in space accomplishments. Certain implications may be directly related to aspects of a specific social environment.¹

II. THREE CATEGORIES OF SOCIAL PHENOMENA

However, though we assessed the list of social problems presented in the Brookings Report and added to it, even such a full listing was far from our task of determining and describing feasible social science research projects which the Committee could sponsor or undertake.² We determined, therefore, that our next step was to group these social problems into significant broad categories of social phenomena; categories in which meaningful and feasible social science research could be designed and undertaken. We must admit, at the outset, that our choice of categories, and subsequently of specific projects and research designs, was influenced by our own research abilities and theoretical interests.

The remainder of this chapter describes and discusses the space program as a social phenomenon in three broad categories we consider significant:

- A. the space program is part of a wave of massive technological innovation;
- B. the space program represents the fusion of an unusually large number of different purposes and interests;
- C. the space program will induce many changes in the organization of our society.

-
1. Michael, Donald N., et al, Proposed Studies on the Implications of Peaceful Space Activities for Human Affairs, prepared for the National Aeronautics and Space Administration by the Brookings Institution, U. S. Government Printing Office (1961), p. 1.
 2. As part of our work in gathering a list of actual or potential social problems engendered by our space efforts, we surveyed members of the Academy by means of a letter eliciting their comments. For a classification and discussion of the replies, see Geno Ballotti's "Analysis and Summary of Responses from Fellows of the American Academy of Arts and Sciences Concerning the Space Program as a Subject for Study and Thought."

A. The Space Program as Massive Technological Innovation

Our national expenditure of vast amounts of physical resources and human energies on space efforts represents the latest phase of a continuing pattern of technological innovation begun with the "Industrial Revolution." And the increasing amount and rate of such innovation since the beginning of this century has been further accentuated in recent years by the entry of the Federal Government, with its immense resources, into the research arena for purposes of national defense and prestige. We need not recount here the many traditional examples of the way in which new technology--the internal combustion engine, for example--can change a whole society.

Social scientists have always been interested in such matters as social change, the reciprocal interdependence of technology and other aspects of a culture, and the integration of new ideas into old ways of life. Yet the massive scale of our current investment in research to generate new technology redirects these issues to the social scientist's attention with new urgency. While one can at first believe that the changes that could result from this massive technological investment will merely be similar to those of the past and differ only in quantity, this very quantitative difference may turn out to be a qualitative distinction of importance.

For example, not only the rate of change but the depth to which changes penetrate our lives could be affected by the enormous scale of our present undertakings. These events may, perhaps, require a radical change in the "every-day thinking" of a large proportion of our population. They may pose so many new problems that old theories and ancient practices of coping with change may be rendered wholly inadequate.

Finally, the very immensity, and organized concentration of the space program and other attempts to achieve massive technological innovation raise some questions and research possibilities never envisioned seriously before the present. For example, how can change in one sphere (space technology) be made to redound to the advantage of the whole social system (civilian economy)? Or, looking beyond our concern with how our own society can survive accelerated change stemming primarily from invention, how can such changes further the actualization of the hopes of all mankind?

B. The Space Program as the Focus of a Number of Different Purposes and Interests

One can easily cite a number of different interests that can be fulfilled through our space program. For example, it helps build our international prestige, accomplishes scientific and military research in outer space, contributes to the defense of our nation, provides a vehicle for injecting funds into the economy to maintain employment and "growth," stimulates research and technological innovation as another contribution to economic growth, and so forth. Any such program can move towards its goals especially rapidly and can exert a profound influence on our society when it mobilizes so many individually powerful influences and purposes into a constellation that will support its vast and diverse activities. And one must understand the elements of such a constellation in order to be certain that the program is best fulfilling its multiple goals.

In order for the goals to be evaluated properly and managed accurately, each of the space program's supporting purposes must be studied in the light of the other interests so that the power and direction of their combined action can be evaluated. And this study must be as broad and early as possible, so that potential wrong consequences are not perceived too late. Conservation laws passed long after field and forest were ruined are testimony to the futility of belated study, understanding, debate and action of our goals and purposes.

The space program represents a unique joining of previously differently associated interests even in the very form of the official organization of the space effort: military joined with civilian; scientist joined with the administrator, and business joined with government. We know little enough in general about the organization of different interests in an effective organization to achieve complex purposes; but we do know enough to recognize that further study would more than amply repay its costs in terms of creating an organization that would most effectively achieve its goals.

In sum, the space effort is dependent on men, organization and material resources. Men and organizations can be better directed toward the achievement of multiple goals if we know how these goals relate to each other, and what reflective moral questions we must raise about these goals. In sum, we are saying that in the implementation of massive technological enterprises the "moral" plays at least as significant a role as the material.

C. The Space Program as Initiator of Changes in the Organization of Society

Here is the category into which most of the numerous examples of social phenomena engendered by the space program could be placed. Any massive technological enterprise such as the space program will have a diversity of unplanned effects on the organization of society. And social science research can increase our skill at anticipating, adapting to, or making provision for such unplanned effects.

One much discussed example of the obvious unplanned effects which may flow from the space effort is the possibility of a shortage of skills in certain key areas of engineering, education, and even basic science. In effect, by drawing today's manpower into space research we may actually reduce the number of persons available to train the future generations of scientists and engineers, thereby threatening the success of the space program and the achievement of other national goals. Consequently, we must determine whether the threat of such a shortage is real, how great, how likely, and if so, what means exist to prevent or ameliorate the shortage? For example, could we make greater use of skilled women, extend the working age limit, rehabilitate and use older persons part-time, develop different methods for training skilled workers and engineers, improve management technology, adopt better techniques of information retrieval and dissemination, find ways to build morals and stimulate more first-class work, or develop management programs which reduce worker turnover?

This is merely an example of the many different kinds of changes in society that may result from or be required by the space program. Perhaps the most valuable contribution of scholarly analysis might well be to anticipate what would otherwise be unanticipated problems, and to suggest means of dealing with these problems.

III. CONCLUSION

As we have noted, a full listing of social problems is not equivalent to a listing of significant, meaningful and feasible social science research projects. The most important reason for this is that the boundaries of desirable social science research projects cannot be neatly defined by naming social problems. Our own grouping of social problems into three categories of social phenomena underscores and illustrates this point.

Yet even our categories of social phenomena may turn out to be less significant than we see them at present, for in all branches of knowledge, some of the most creative work will center around new and unpredictable ways of looking at events; ways which put together phenomena previously regarded as unrelated. Accordingly, some of the most significant research will "take off" at a level of generality or in a framework which does not specify a direct connection with any named social problem or current concern.

Similarly, though our categories of social phenomena are meaningful, they are far from a listing of feasible social science research projects. And the major difficulty remaining in designing such research projects is the nature of social science research itself. Putting this most simply, a large number of important social problems or categories of social phenomena are not easily researchable by present social science research techniques and methods, or at best, require a great deal of planning time to devise worthwhile social science research. The limits set on designing research projects by the nature of social science research are discussed in Chapter Three of this report.

CHAPTER THREE:

THE NATURE OF SOCIAL SCIENCE RESEARCH

I. INTRODUCTION

In Chapter Two we pointed out that our categorization of the space program as a social phenomenon is not the listing of meaningful and feasible social science research projects that the Committee on Space Efforts and Society might undertake or sponsor. And we noted as well that the major difficulty in designing such research projects is the nature of social science research itself. That is, a large number of important social problems are not easily researchable by present techniques and methods, or, at best, require a great deal of planning time in order to devise worthwhile social science research on them. Consequently, both in order to design research, and, more importantly, to explain this research to our sponsors, we found it necessary to answer the following question: How does the nature of social science research affect the study of the space program as a social phenomenon?

II. NOTES ON SOCIAL SCIENCE RESEARCH

Our goal in the remainder of this chapter is to bring into brief view those diverse characteristics of social science research that we believe affect the design of research on the space program as a social phenomenon. This presentation is by no means a comprehensive or schematized discussion of the nature of social science research as it relates to social phenomena generated by the space program. Rather, the notes that follow serve a limited, two-fold purpose: first, to acquaint the Committee with our views on the nature of social science research and, thereby, how these views influenced our recommendations and choice of suggested research projects; and second, to provide the Committee with a view of the nature of social science research which, we believe the Committee must transmit to the National Aeronautics and Space Administration (NASA) so that NASA can better understand and evaluate the social science research presented to it.

A. Many interesting social problems are not readily researchable by present social science research techniques and methods.

For example, many social problems cannot be readily identified because techniques of mass observation are not available to the social scientist, and years of lead time would be necessary to develop such reliable techniques. The existing techniques of social science research have not been perfected to the point where they are readily and reliably usable in studying rapidly a large volume of social phenomena. Similarly, individual techniques have not been used in a wide enough variety of different analytical situations so that one can readily adapt them to new studies. In effect, each new social science research project requires to a good degree the development of new techniques or the special tailoring of existing methodology. This "experimental" nature of social science research adds to the need for increased lead time in designing projects, and limits the routine use of social science research.

To help overcome this limitation, social science badly needs a device midway between itself and journalism that can furnish it with crude data, moderately reliable observations, and materials likely to generate hypotheses for more detailed test later. Some modification of "mass observation" might well fill the bill.

- B. The boundaries of desirable social science research projects cannot be neatly defined by naming a social problem.

As in all branches of knowledge, some of the most creative work will center around new and unpredictable ways of looking at events, ways which "put together" phenomena previously regarded as unrelated. Accordingly, some of the most significant research will "take off" at a level of generality or in a framework which does not specify direct connection with any named social problem about which we are not currently concerned. This spontaneous reorganization of information into new ideas or theories --call it insight if you like--is an important characteristic of social science research, and accounts for, at least in part, the lack of relation between a researcher's original intention and his findings, as discussed below (C) .

- C. There can be only the loosest of relations between the original intentions of an investigator and the actual outcome of a given social science research project.

In the present state of social science research, an original intent to search for the answer to a very particular question may have as an outcome an answer to a very general question, and vice versa. This looseness of the intention-result relationship in social science research must be accepted as a fact of life for the indefinite future. Consequently, it is very difficult to develop a successful social science research strategy based upon the declared intentions of the investigators.

- D. Good social science research is characterized by the way in which it is carried out and by its being reported in such a fashion that its most general implications can be inferred readily.

For example, pigeon-pecking and dog salivation can be studied and reported in such ways as to illuminate or raise vital further questions about all learning, perhaps even about all behavior. Similarly, one cannot guarantee good results from social science research by demanding that the research problem be stated in advance in given general or specific terms, for, as noted above (C), there is no direct relation between the researcher's original intent and the actual outcome of the research. Rather, a good outcome from social science research is guaranteed by the breadth of interest, knowledge and competence of the investigator himself, and certainly not guaranteed by the apparent breadth or narrowness of what he first believes he wishes to study.

- E. Social science research can be conducted on any scale of money or manpower.

And there is no direct relationship between the scale of the research and the value of the findings from that research. However, it is our belief that a "less-than-sufficient" scale of social science research may well result in waste of social science resources, if only because such limited research may make visible one piece of a jigsaw puzzle not intelligible without the others. No one knows exactly

what is an adequate and feasible scale of social science research for a project as large as our space efforts, for example, but our best judgment is that one mill of every operational dollar expended by NASA is an appropriate rate at which to begin.

- F. Choices between "guided" and "unguided" social science research projects should be made on the basis of the competence of the investigator.

We define "guided" research as those situations in which the selection of the problem and the major research strategy in its investigation lies with someone other than the principal investigator. We define "unguided" research as the absence of such direction or control. Research sponsors usually believe that they must make the choice as to whether to guide or not to guide the project design in order to insure that some findings result from the research. However, the important issue is not whether to minimize or maximize such guidance solely in terms of the amount of help given, but rather, to tailor the amount and kind of guidance to the ability of the investigator to work without guidance. In sum, each investigator, in so far as possible, should be given the degree of freedom that he can best use and tolerate.

- G. More significantly, there simply are not enough qualified people able and willing to undertake serious social science research in new areas.

Most qualified persons are already committed to definite career lines or interests.¹ Consequently, it will be very difficult to lure them into new projects. This shortage of social science manpower becomes particularly important when we realize that, almost certainly, the space program and other programs of massive technological innovation will continue to generate social problems, all at present substantially unanticipated, at an unknown rate.

- H. If the social sciences are to be adequate to the research needs proposed in this or any other report, then they must develop better means of information retrieval and dissemination than currently in use.

Social scientists do not have available to them at present easy access to the great volume of research already done, to the great amounts of data gathered by unanalyzed, to the data available but ungathered, and the like. A drastic improvement in information retrieval and dissemination is a prerequisite to improving the efficient use of social science research dollars.

- I. There is no special, sovereign virtue in either single discipline or "interdisciplinary" social science research.

What needs encouragement, however, is a multiplication of opportunities for persons with different formal educations to attempt to communicate with each other. Such

1. See Chapter Four.

communication is especially urgent between the social sciences and the humanities, and the social sciences and the natural or engineering sciences.²

- J. It is time for social scientists to carefully think through the essential purpose of social science research, both for the sake of social science and the "users" of social science.

That is, to what degree, if at all, should the purposes and goals of social science research be determined by the aims of financial, social, administrative or other sponsors? For example, should the sponsors of social science research use it to prove a need for social action? To what degree is social science research open to other forms of intervention?

The ethics and other aspects of the consumer-producer relation in sponsored social science research require study, discussion and definition. In effect, we ask who is a "valid" sponsor for social science research into a given area? What limits, if any, can the sponsor set on the research to be done? Another side of the same coin is the answer to this question: What agency, person, institution, or the like should hold itself responsible for initiating and sponsoring what kinds of social science enquiries? No ordered discussion of these most vital problems, in so far as we know, exists, and yet such a discussion and the consequent definition of moral boundaries should precede the further development of social science research.

- K. Social science research need not be oriented specifically toward policy findings in order to have policy implications.

Formulating research problems initially in terms of specific policy needs will not automatically yield findings useful in determining policy. What distinguishes "good" research in this area is the ability of someone (not necessarily the original researcher) to draw policy implications from any kind of data, whether the original research was formulated in terms of specific policy needs or not. Consequently, research sponsors need not attempt to balance their programs between "policy oriented" and "theory oriented" research in order to insure findings that will have policy implications.

In fact, such policy interpretations are so much a function of the talent of the individual interpreting the data that the process of drawing policy implications might well be profitably separated from research into a recognized, conceptually independent enterprise analogous to "development" in the Natural Sciences. Unfortunately, given the history of social science in the past fifty years, emphasis may well have to be placed on legitimating policy-oriented research, since most manpower is attracted to "theory orientation" as being undilutedly praiseworthy.

- L. The closer social science research comes to examining vital questions of policy, the more it will be impugned as being "controversial."

Similarly, the more profound and probative the social science enquiry, the likelier will it be to be attacked as "irrelevant." All persons participating in social science

2. See Chapter Four.

research of any consequence must be prepared to hear such attacks, and must learn to discount the attributions and continue to carry forward the research as originally designed. However, it is the researcher's responsibility to see to it that the strategy and its foundations are understood by others to begin with, so as to decrease the likelihood that the research may have to be altered to meet ill-founded criticism.

III. CONCLUSION

The portrait of social science research presented in this chapter emphasizes its diffuse, unboundaried, "experimental" nature. We have pointed out that the boundaries of social science research projects cannot be neatly defined by merely naming a social problem, and that many social problems are not readily researchable by present social science research techniques. In sum, we are stating that the tasks of formulating the research problem and devising the research techniques are far from easy or routine in social science research.

Similarly, we have emphasized that there is only the loosest of relations between the original intentions of an investigator and the actual outcome of his project, and that "good" social science research is characterized by the way in which it is carried out and the manner in which it is reported. In effect, we are stating that the most important element in obtaining "good" research is the ability of the researcher--his talents, his breadth, his energy.

We went on to discuss a number of currently accepted generalizations about the management and formulation of research: the scale of research, the "interdisciplinary" approach, the manner in which research problems are formulated and projects are guided, and many other issues. Again, we conclude that most of the decisions on these issues or problems must be made in terms of the specific research project and the capacities of the researcher.

If our description of the nature of social science research cumulates to any one emphasis, it is an emphasis on caution. The researcher must be cautious when designing social science research. The sponsor and researcher must be cautious when evaluating research designs and strategies so that their own preconceptions do not prevent them from seeing the essential values of the project. The sponsor must be cautious when supporting research, so that he does not expect greater, more permanent, or more perfect findings than social science research can generate.

This emphasis on caution, on the "experimental" nature of social science research, leads us to formulate our most urgent plea concerning the study of the space program as a social phenomenon: good research--valuable research--takes time to plan, and the wise sponsor will plan his research program to allow for that time. Significant research projects on the space program as a social phenomenon should be planned and financed to allow for a long lead time. The planning and discussing of major projects should take many months, perhaps even years, before men, money, energy, resources and good will can be risked confidently on important field studies.

An understanding of the need for lead time, and allowing for it, will help reduce the conflict between those who believe that research on human affairs should emphasize reflection, speculation and analysis, and those who insist on empirical research, on "doing something." Adequate lead time will permit joining these two approaches in social science research far more than is usually the case.

In conclusion, we believe that the nature of social science research is such that the decisions of the sponsor--his goals, attitudes and predispositions--will most importantly affect the study of the space program as a social phenomenon. This concentration of influence raises anew the ethical and policy issues discussed in this chapter (see II:J, K and L). And these issues in turn were relevant to us when we were designing the research we recommend later in this report. To design this research, we had to formulate what we believed to be the proper demands and roles of the two sponsors, NASA and the Committee on Space Efforts and Society. We present these formulations as parts of Chapters Four and Five. Chapter Four, our discussion of the role of NASA, precedes our presentation on the role of the Committee on Space Efforts (Chapter Five) because we believe that a formulation of NASA's role was an important background component when we determined the role of the Committee and the research program we recommended to it.

CHAPTER FOUR:

THE ROLE OF THE NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

I. INTRODUCTION

We envision that initial support for research on space efforts as social phenomena will come largely from the National Aeronautics and Space Administration (NASA). The decision to support such research raises two questions. First, what general types of research and specific research projects should be underwritten? Second, what relationship should NASA have, as sponsor, to those persons or institutions carrying out the research and to social science research in general?

The first question essentially asks that we determine what research would be "useful," "valuable," or "urgent" to NASA. As we pointed out in Chapter Two, this task requires more than a priority listing of social problems worthy of investigation. It requires a meaningful grouping of social problems into broad categories of social phenomena so that the more urgent or worthwhile of these categories can be presented as demanding immediate or first priority study. We list three such categories in Chapter Three. In Chapter Five we will put forward our suggestions on the standards to be used in designing or evaluating specific research on space efforts as social phenomena. Chapter Five also contains our recommendation for a general research strategy and for specific research projects that the Committee can undertake itself or can recommend to NASA.

The second question is the topic for discussion in the remainder of this chapter. Most of our discussion in Chapter Three on the nature of social science research has a very direct bearing on the role we envision for NASA as a sponsor of such research. For example, whenever social science research bears on policy matters, it is very likely to provoke controversy, for such research will often embarrass someone by raising questions that for the immediate purposes of day-to-day administration might be better left unasked. Another source of controversy is that social science research is apt to appear complex to those not accustomed to it, and its results are likely to be seen as "obvious" by those who agree with them or to be felt as irritating and "irrelevant" by those who do not. Most importantly, any given administrator or agency has a tendency to desire immediately useful research, even when there is an initial commitment in a broader direction. All of us have had sufficient administrative experience to predict with a high degree of probability that such a demand and desire will come from NASA eventually in one manner or another. Consequently, the relation of the sponsor to the research is important, for this demand not only raises the issue of bias but also whether the resulting research accomplishes anything worthwhile.

As persons with some experience in the study of human affairs, we point out again that the nature of social science research is such that the most significant contributions

which it can make to NASA and our nation will be achieved by concentrating on that work which contributes to organized knowledge as a whole, regardless of the immediate utility of that work. And we also want to emphasize that the tendency which exists on the part of most grantors--naturally and understandably--to emphasize practical results, demands that social scientists act as a countervailing force and vocally call attention to those philosophically and scientifically significant issues which are of no practical or immediate value, lest such issues be overlooked altogether.

In sum, we emphatically recommend that the National Aeronautics and Space Administration sponsor social science research in such a manner as to encourage broad, unfettered inquiry into the nature of space efforts as social phenomena.

The implementation of this policy requires a solution to two problems. First, how does NASA go about generating sustained social science interest in studying the space program as a social phenomenon? And second, how does NASA, both organizationally and operationally, go about sponsoring broad, unfettered social science research enquiries?

II. GENERATING SOCIAL SCIENCE INTEREST IN SPACE EFFORTS

A reader should have no doubt by this time that we believe the space program worthy of extensive social science study as a social phenomenon. Unfortunately, the context within which NASA will undertake a social science research program is one of general lack of manifest interest in space matters on the part of social scientists.¹ When one couples this lack of interest with the shortage of skilled social science manpower described in Chapter Three, it is all too apparent that NASA will have to work hard to interest social scientists and to draw them away from the research areas to which they are already deeply committed.

Consequently, if NASA is to proceed with an effective social science research program, it must develop and maintain rapport with the social science community. A first step in this direction is to disseminate space information among social scientists.

At present, space does not fit neatly, conventionally, or conveniently into the interests of most social scientists, nor do social scientists see how the problems generated by space efforts are related immediately or directly to customary social

-
1. An example of the lack of interest in doing research on space matters which strikes us as fairly typical is supplied by the following letter: ... "After some soul-searching, I have decided not to accept your kind invitation to write an essay on social indicators. As I thought about secondary consequences of space exploration, it dawned on me that one of them was that a specialist with a great deal of work to do was being diverted from his chief concerns. I also came to appreciate that I am pretty thoroughly antipathetic to our space program, particularly on its present mammoth scale, with its space race and cold-war associations, and with its ill-defined scientific goals. Thus, I could hardly qualify as a dispassionate observer."

science research. However, the more social scientists know about the space program and the scope of space efforts, the more likely it is that specialists in various disciplines will perceive implications for their work or areas for worthwhile social science enquiry.

A number of different methods for accomplishing these educational goals come to mind. For example, NASA could include social scientists as consultants in the planning, undertaking and evaluating of various space programs. These men would, in turn, relay their experiences and impressions to their colleagues through regular professional communication channels. Social scientists employed within NASA would serve this same function. Similarly, NASA might educate social scientists by including them at natural science and engineering conferences, or by encouraging face-to-face meetings between NASA personnel and social scientists. In addition, NASA might attempt to generate social science interest by asking social scientists to discuss the ways in which the space program resembles and differs from previous historical events, and, subsequently, preparing an "Analogy-Disanalogy List."

It is equally important that a few NASA officials keep abreast of social science activities and ideas, so that they will have a common "universe of discourse" with the social science community. This goal can be achieved in part by hiring social scientists to work within NASA (see part III-B). In this way, NASA will be able to recognize more readily what aspects of its activities are most relevant to the social sciences, and know the terminology to be used when communicating space program information and NASA research needs to social scientists. Such an awareness of social science viewpoints will facilitate communications between NASA and social scientists, and, most of all, will encourage social scientists to consider doing research on space efforts as social phenomena.

Finally, we wish to stress here that social scientists are relatively unaware of the engineering and development fields, especially when compared to their knowledge of the basic natural sciences. Since many of the opportunities and problems of NASA, and all massive technological efforts, are engineering-development oriented rather than basic science oriented, we believe that it is of real importance that social scientists learn more about such disciplines and their methods for thinking about problems. We believe that NASA should sponsor such education, and in this way encourage interest in space efforts through fostering a better understanding by social scientists of those disciplines (and their practitioners) which underly our space efforts.

Once rapport is established between social scientists and NASA, NASA must organize so as to generate and sustain continuing interest in space programs and their study as social phenomena. To do this, specific programs must be devised, and their principle objective must be to establish an on-going working relationship between NASA and the social science community, a relationship that is based upon mutual respect and concerned with the advancement of the social sciences in general. Part III of this chapter will describe three methods for achieving this goal.

III. SPONSORING BROAD SOCIAL SCIENCE RESEARCH ENQUIRIES

While the four methods of sponsoring or encouraging social science research on space efforts discussed below are not the only methods that could be suggested or devised, we believe that they all work to implement the goal emphasized earlier in this chapter: that NASA should sponsor broad, unfettered social science research. The most significant of these methods, unrestricted grants, is discussed immediately below, followed by our suggestions concerning in-house employment of social scientists, contractor employment of social scientists, and the endowing of university chairs and sponsoring of special seminars and conferences.

A. The Need for Unrestricted Social Science Grants

We believe that the most effective method for supporting broad, unfettered social science research is through giving unrestricted long-term grants to research institutions, individuals, and universities. Our arguments for this position follow.

1. Unrestricted grants guarantee the long lead time required for good social science research.

Our complete discussion and reasons for making this assertion can be found in Chapter Three. Briefly, however, given the nature of social science research and our belief that there is often only the loosest of relations between the original intentions of an investigator and the actual outcome of a given social science research project, we conclude that good social science research is characterized by the way in which it is carried out and by its being reported in such a fashion that its most general implications can be readily inferred. Consequently, the "experimental" nature of social science research places great emphasis on the abilities of the researcher and on planning and thinking through of the research in advance. In sum, good research takes time to plan, and the wise sponsor will plan his research program to allow for that time. An unrestricted long-term research grant removes much of the sponsor's pressure on the researcher to study certain specific problems within a given length of time. Such grants allow for extensive lead time and free the institution or researcher to explore the topic area, within mutually agreeable limits perhaps, and thereby encourage broad enquiry.

2. Unrestricted grants free the researcher from administrative interference on his abilities and imagination.

Many times a researcher will find it intellectually valuable to redefine his problem or revise his research techniques after having completed some or even a considerable amount of research. Such redefinition may result from seeing new relationships in his data, or from new information generated by the research itself. Often, "outsiders"--even other social scientists--will not understand or will be confused by this reorientation, and his sponsors may demand that the researcher either return to his original strategy or spend time explaining and justifying his new definitions or techniques. Unrestricted grants encourage the researcher to pursue the thinking he deems intellectually valuable and free him from being concerned about explaining all details to his sponsor. One by-product of these grants, then, is that additional time will be spent on research rather than on purely administrative matters or in "educating" the sponsor (see 3 below).

3. Unrestricted grants reduce the amount of time and funds spent on purely administrative matters.

Our experience leads us to conclude that administrative and other non-productive time tends to be considerably higher in good short-term research operations than in good long-term operations. This is usually because such short projects have greater difficulty in securing and keeping qualified auxiliary personnel, both because they lack employment security and because short-term grants are usually concerned with research topics having limited professional potential. In addition, more time is spent, proportionately, in dealing with the sponsor (see 2 above) on short-term grants. Long-term grants certainly provide more employment security, and long-term unrestricted grants also provide more opportunity to achieve personal goals of professional development, thereby attracting the most competent and energetic researchers. Accordingly, the most desirable research results may be achieved by giving very long-term, unrestricted grants to qualified investigators, providing them with facilities, secretaries and the like over a period of years, and allowing them to work in the area of their interest.

4. The research results from unrestricted grants are usually viewed as being less "biased" than other sponsored research findings.

In many respects this is an important by-product of unrestricted grants. The importance of this appearance depends, of course, on the use to which the research itself will be put by the sponsor. But, as noted in Chapter Three, part of the persuasiveness of the findings is based on their appearing independent of interference from the sponsor--and unrestricted grants would certainly appear more unbiased than closely directed, specific, short-term projects.

Obviously, if NASA is to make such unrestricted grants it must have some method of preventing these funds from being spent wastefully or on research completely unrelated to space efforts. We would like to suggest an indirect and tasteful method for doing this. As a condition in such a grant, NASA should require that the grantee spend a certain period, probably a month, each year actually exploring and acquainting himself with space installations and space activities. The actual assignment would be subject to the discretion of his NASA liaison administrator. In addition, NASA could call on him for up to twenty working days every two years as a consultant on any NASA or NASA-related program. Such activity and involvement, we believe, will lead scholars to search for examples and specific problems illustrating their general research concerns, and that their curiosity will be aroused about social phenomena connected with the space program.

B. Employing Social Scientists in NASA

We suggest that NASA employ social scientists in those parts of its operation where their training and background can be used. For example, social scientists could do worthwhile work on long-range planning, international relations, manpower utilization, training astronauts and space explorers, public relations, economic and budgetary analysis, and applications work. In this way NASA would build into its organization men who understood the usefulness of social sciences and who, in turn,

could both perform limited social science research in their operational area and supervise, arrange for, or encourage social science research by others outside NASA. These men would serve as a channel of communication about NASA activities to social scientists (see II above) and would thereby help generate research interest about social phenomena connected with the space program.

We are not suggesting, however, that such activities be regarded as the particular prerogative of social scientists. And we most emphatically oppose the establishment of any particular social science facility or division within NASA. "Basic" or significant social science research cannot be done as satisfactorily on an in-house basis as by organizations outside NASA, for all the reasons discussed in Chapter Three and above in part III-A. In addition, no operational problem within NASA that we know about is the chief, exclusive, or sole concern of social science rather than some other discipline. Finally, the giving of grants is best done as part of some general procedure, using social science advisors as consultants, rather than as the research extension of an internal NASA division.

C. Employing Social Scientists in Contractor Operations

For all the reasons presented above, contractors for NASA--for example those concerned with systems development, space exploration, applications, and the like--are likely to deal with problems which are relevant to social science analysis. In these areas, at least, NASA, through its contracting and monitoring procedures could persuade contractors that social scientists would make useful contributions both as regular employees and as consultants.

D. Establishing University Chairs and Sponsoring Seminars

In order to develop lasting scholarly interest in space efforts, NASA should endow university chairs, and sponsor seminars or conferences. Out of such efforts could come curricula designed to familiarize social science students and teachers with space efforts and their study as social phenomena, as well as greater familiarization among natural scientists and engineers with social science. The end product of such programs should be cadres of sophisticated personnel who will be invaluable to future social science research on the relation of space efforts to society.

IV. CONCLUSION

We believe that, by sponsoring social science research in such a manner as to encourage broad, unfettered enquiry into the nature of space efforts as social phenomena, the National Aeronautics and Space Administration will best achieve its own need for excellent social science research and will, at the same time, best benefit the development of the social sciences in general. We found this belief on our evaluation of the nature of social science research, an evaluation which led us to conclude that "good" social science research is usually the result of the talents of the

researcher coupled with his being given freedom from restriction and ample lead time to formulate problems and research strategy. This conclusion, when added to our evaluation of the usual effects of sponsor demands upon research and researcher alike, causes us to propose that NASA's best vehicle for achieving the goals stated above is to sponsor social science research through unrestricted, long-term grants and other similar methods. We note as well that in order to generate social science interest in studying space program generated social phenomena, NASA will have to overcome current social science apathy and lack of knowledge about space efforts. This chapter contains suggestions to help generate such interest.

We realize, of course, that implementing our suggested role for NASA as sponsor of social science research will require two important elements: organization and money. NASA will have to establish proper administrative machinery within the Agency to accomplish each aspect of the social science research process: definition of sponsor-researcher roles and functions; screening of requests; allocation of grants; research requirement planning; research liaison; monitoring; and most importantly, utilization of research findings. We believe that NASA itself knows more about locating such administrative machinery within the Agency than could any outside organization. Consequently, other than our firm recommendation that no special social science division or unit be established, we refrain from recommending specific administrative forms and devices within NASA.

As for funding, we repeat an evaluation and recommendation made in Chapter Three. Social science research can be conducted on any scale of money or manpower. And there is no direct relationship between the scale of the research and the value of the finding from that research. However, a "less than sufficient" scale of social science research may well result in waste of social science resources, if only because such limited research may make visible one piece of a jigsaw puzzle not intelligible without the others. No one knows exactly what is an adequate and feasible scale of social science research for a project as large as our space efforts, but our best judgment is that one mill of every operational dollar expended by NASA is an appropriate rate at which to begin.

Though we envision that initial support for research on space efforts as social phenomena will come largely from the National Aeronautics and Space Administration, we believe that it is extremely important for a substantial proportion of research in this area to come from private foundations, other government agencies, international organizations, and countries not currently contemplating serious participation in space efforts. Even if, through unrestricted grants and other such devices, a good deal of NASA-financed research should be completely objective and uninfluenced by the inevitable interest of the grantor in defending its program, an old principle of equity applies here: the research must appear impartial. This appearance of impartiality is far more likely when research is financed from different quarters, by sponsors believed to have different preconceptions.

Some of these issues will again be discussed in Chapter Five, which is a discussion of the role of a NASA-financed research group: the Committee on Space Efforts and Society. The chapter contains a discussion of the standards to be used when designing or evaluating specific research on space efforts as social phenomena, as well as our recommendation for a general research strategy and specific research projects that the Committee can undertake itself or recommend to NASA.

CHAPTER FIVE:

THE ROLE OF THE COMMITTEE ON SPACE EFFORTS AND SOCIETY

I. INTRODUCTION

Throughout this report we have emphasized that the "experimental" nature of social science research and its consequent dependence on the talents of the researcher make it imperative for both sponsor and investigator to allow ample time for problem definition and strategy design. Good research takes time to plan, and the wise sponsor will plan his research to allow for that time. In Chapter Four, we embodied this philosophy in our suggestion that NASA use unrestricted, long-term grants when sponsoring social science research. In this chapter, we will apply the philosophy to the role of the Committee on Space Efforts and Society as a sponsor and doer of social science research.

Specifically, we believe that the Committee on Space Efforts and Society should plan to operate and sponsor projects for a four to five year period in order to produce sound and significant social science research. That is, more time than the two years of the Committee's grant is necessary to plan and carry out sound research projects, and more money than is available in the Committee's grant is needed to finance significant projects. We sincerely believe that little meaningful research will result from operating over a short period with restricted funds, for our experience indicates that such limitations frequently produce hastily designed and uncompleted projects. We recognize, of course, that the Committee will have to gather additional funds both to operate over a longer period of time and to sponsor more extensive research.

We believe that the limited funds of the Committee and the short time period that it currently proposes to be in operation require a choice among the following three alternatives:

- A. to confine Committee activities to surveying those forms of social science research and philosophical inquiry relevant to massive technological activities such as the space program; or
- B. to engage in preparatory activities such as bibliographies, research plans and pilot research; or¹
- C. to raise, by application to NASA or elsewhere, the funds necessary for a four to five year program of supporting specific extensive research projects.

-
1. Comment: Such activities will remain uncompleted at the end of the Committee's currently planned period of operation, and will require great skill in selection and monitoring to make them as worthwhile as a single larger project.

We emphatically recommend this last alternative. We are not suggesting, however, that the Committee commit itself and the American Academy of Arts and Sciences to a permanent operation. Rather, we believe that there is currently a need to demonstrate the value of and opportunity for social science research on the social phenomena generated by the space program. By operating over a four to five year period, the Committee should be able to sponsor and complete enough significant research for this need to have been shown. At the end of five years, we envision that the Committee's pioneering efforts will have stimulated enough independent inquiry for the work to be continued by others, and the Committee could regard its mission as completed.

In the remainder of this chapter we present our suggestions on the standards to be used in designing or evaluating specific research on space efforts as social phenomena, together with a general research strategy and specific research projects that the Committee can sponsor, undertake itself, or recommend to NASA.

II. NOTES ON DESIGNING RESEARCH ON THE SPACE PROGRAM AS A SOCIAL PHENOMENON

In Chapter Three, "The Nature of Social Science Research," we presented what we consider to be the characteristics of "good" social science research. All of these characteristics should be included here as general standards to be used when designing or evaluating research on the social phenomena generated by space efforts, but we will not repeat them. In addition to these general standards, we believe that a number of specific criteria should be applied to the study of the relation of space efforts to society.

A. Social science research on the space program as a social phenomenon requires two activities, search and planning, prior to the actual undertaking of a research program.

By "search" we mean two quite different but connected activities: first, an orderly review of the tools and conceptualizations of the social sciences in their own right and in terms of their capacity to cope with new problems; and, second, an orderly review of productive or promising methods of formulating what are the problems of (and for) the space effort. By "planning" we mean the development of a general research strategy based upon the search, paying attention both to the needs of social science and of the space effort, and appropriate to the tools and methods available or engenderable.

B. Persons doing research on the space program as a social phenomenon should understand and be able to draw meaningful inferences from the ways in which previous historical events are analogical to the space program.

For example, the space program represents state sponsored exploration just as did the voyages of Columbus. Similarly, the space program is analogous to other programs of massive technological innovation, to previous peaceful focuses of national purpose, and to other forms of government-private joint enterprise. Such analogies should be helpful in understanding and foreseeing impacts of space efforts on our society.

- C. Research on the space program as a social phenomenon should be concerned with immediate, intermediate and long-range impacts and effects.

It is not desirable to define a strategy that would allow research to focus on only one or two of these time dimensions, since each of the three could be of overriding importance. We do offer the comment, however, that urgency and practical necessity usually create sufficient pressure to make one attend to immediate issues; and that we must take care not to neglect intermediate or long-range impacts because they are less clamorous. These future events are equally (or more) important, and they must be foreseen and receive adequate research attention.

- D. Similarly, research on the space program as a social phenomenon must investigate the immediate, intermediate and remote impacts and repercussions on our social structure.

In terms of social structure, impacts on the immediate working habits of those directly involved in the space effort lie at one end of the spectrum, while matters such as changes in general public attitudes toward science lie at the other. None of the intervening categories of impacts on social structure should be ignored, although, again, we must guard against the natural disposition to give most or first attention to immediate impacts. This need for a wide range of study applies to geographic impacts as well.

- E. All levels of analysis--the person, small group, community, institutions or society--are worthy of study as part of research on the space program as a social phenomenon.

We do, however, recognize that at least one priority for study should be established. Immediate research attention should be given to those institutions on which the space effort depends most heavily: the educational system and the research system.

- F. Research on the space program as a social phenomenon should give special attention to the blurring by time and events of what once were sharp, dichotomous distinctions in our society, such as: public-private; governmental-nongovernmental; basic-applied; scientific-nonscientific.

- G. "Transactional" studies are of crucial importance in a well-conducted program of research into the space program as a social phenomenon.

That is, research should examine the reciprocal effects on each other of the "systems" studied, rather than the unilateral effect of one system cast as an "independent variable" on the other. For example, we need to know not so much about the "impact of massive technological innovation on society" as about the interactions (reciprocal effects) within one social system of those elements most immediately and directly affected and those not.²

2. See Bentley, A. F., An Inquiry into Inquiries, Beacon Press, 1954.

- H. An adequate social science research program on social phenomena generated by the space program would have as a by-product the development of more effective means of information retrieval than those presently available to the social sciences.

As noted in Chapter Three, the current methods of information retrieval and dissemination in the social sciences are inadequate to the need for knowing about the impact of space efforts. Thus, good research on the space program as a social phenomenon should improve the information retrieval techniques of social science as well as clarify, reconceptualize, and reorder the ideas and alleged facts gathered in the research.³

- I. Research on the space program as a social phenomenon should be preceded by a careful examination of how social science should study engineering and development matters.

While in recent years the interdisciplinary general education movement has familiarized social scientists with physics and physiology, it has done very little to instruct them in engineering and development. In general, social scientists need to learn about engineering and development because many of the impacts of our space program, and all massive technological efforts, are generated by engineering or development activities rather than by basic science work. In addition, we believe that it is of real importance for social scientists to know more about such disciplines in order to learn whether their methods for thinking about problems are useful in doing social science research.

- J. Similarly, engineers and natural scientists should be encouraged to participate in social science research on the impacts of space efforts.

Experience to date indicates that researchers participating in projects using interdisciplinary teams usually find that the members of such teams acquire something of each others' perspectives and patterns of conceptualization. The gravest obstacle to fruitful interdisciplinary cooperation is the notion that one science, one discipline, or one point of view is the master and that the others are simply auxiliary, and the notion that one can divide problems into disciplines is an artificial oversimplification. Consequently, the fresh viewpoints of engineers and natural scientists may contribute much to developing social science research in this new area of space efforts. One of the fruits of our emphasis on adequate lead time for planning would be that the time would be available for fuller cooperation between engineers, natural scientists, and social scientists. This cooperation is especially needed for the kinds of research we recommend later in this chapter, for this research emphasizes the interdependence of the different skills needed to study complex issues.

- K. Other nations and social scientists from other nations should participate in research on social phenomena generated by space efforts.

A number of benefits would accrue from such international cooperation. First, our own research would benefit from the participation of social scientists from other

3. See Gross, Bertram, "Operation Basic," Journal of Communications, 1961.

nations in the study of social phenomena generated in our country by our space efforts. An elementary, yet important and far reaching, tenet of social science is that our way of looking at the world--layman and scientist alike--is structured by the process of growing up in our society. As a result, we see things in ways peculiar to those raised in our culture, and miss seeing things or seeing in ways that others, raised in a different culture, would see. For some purposes--e.g., taking a national census--our selective vision hardly matters. For others--e.g., depicting our society as it appears to an insider--it is a partial asset. But for many purposes--especially for the kind of research we propose--it is a limitation. Of course, the training of any social scientist should enable him partly to discount this limitation. But he can never wholly offset it, and could, consequently, gain new insights from the observations of someone from outside his society.

Second, international cooperation would provide opportunity to study the impact of space efforts in other societies as well as providing for projects sponsored by other nations. We have pointed out elsewhere that the auspices under which research is conducted is very important to an outsider's perception of the value of the research. So very important, in fact, that well planned research under badly chosen auspices may be seen as "biased" or "worthless." In addition, a researcher could be denied access to data because of fear of or dislike for his sponsor. Above all, credibility of findings is frequently as much a matter of the quarter from which an assertion is made as it is of the character of the assertion. This is not just an unfortunate characteristic of social science and its consumers, it is intrinsic to social science. For all these reasons, as well as for the positive opportunities for cross-cultural study available through international cooperation, many projects should be organized by the joint effort of more than one nation--both within and across lines of political ideology.

Third, international cooperation would provide new opportunities to exchange social science theory and techniques among nations. For example--and though it may offer peculiar difficulties--mutual benefit could accrue from social science cooperation between the United States and the U. S. S. R. We have as much to learn from them as they from us. We have to offer, we believe, a generally more advanced social science, particularly in sociology, anthropology and psychology. They have, as far as we know, superior techniques of planning, information funding and retrieval, and the application of social science. It is not impossible that some movement toward a cold war detente could emerge gradually from establishing international institutions which foster communication, cooperation and coordination between the social sciences and scientists of countries on both sides of the walls and curtains.

A primary benefit in better knowledge would alone justify the additional expenditures of time and money that international cooperation would require. But the bonus of international trust, understanding and even, perhaps, amity would more than justify the costs. Indeed, such international cooperation itself would provide a new kind of social science laboratory for gathering data and observations on the ease or difficulty of trans-national collaboration in scientific affairs.

III. CONCLUSION: A WORK PROPOSAL

We propose that the Committee on Space Efforts and Society undertake the following "two-pronged" research strategy:

First, that the Committee support or organize systematic research on a limited number of major research projects. For each of these major projects we recommend prudent preliminary work that will include (a) an appraisal of previous relevant research and related research now in process; (b) a careful clarification of the topics and concepts on which the research is being designed; (c) a formulation of preliminary research hypotheses for the project; (d) where necessary, a preliminary field survey; and (e) the development of a detailed "research design."^{4b} Above all, it is essential that highly qualified people manage and staff each project from inception to completion.

Second, that the Committee support smaller scale, unguided inquiries. Many interesting research topics have not been developed sufficiently to warrant committing large amounts of money to them as major projects. Nor does the Committee have sufficient funds, at present or in prospect, to support more than a few major projects. Yet many such topics would benefit from serious scholarly attention in the form of inquiries, occasional papers, evaluation and thought essays, research proposals and other such limited investigations. A portion of the NASA grant should be reserved for such smaller projects. It is likely, of course, that ideas or research developed in this manner will lead to one or two additional proposals for larger-scale systematic inquiry.

A. Major Projects

We propose five project areas deserving major attention or systematic research. Projects #1, #2 and #3 suggest research on the impact of space efforts on society. They are major projects in terms of the funds required for systematic research, their rele-

4. Comment by the Executive Director: The Committee on Space Efforts and Society decided that, given the limitations of its grant, this work proposal was more ambitious than it could undertake. At the same time, the Committee believed that the work proposal was worthy of wide consideration and discussion. Consequently, it is included in this version of the Summer Study Group Report. --

Lewis A. Dexter, 1 February 1963

- 4b. Some of these preliminary works will represent in themselves worthwhile contributions to knowledge, and, consequently, deserving of separate and seriatim publication.

vance to NASA's present concerns, and their yielding significant social science research. These projects will eventually require more funds than are available from the Committee's present grant. Project #4 recommends a comprehensive examination of the national goals achieved and impinged on by space efforts. This proposal relates to the "national goals" aspect of the Committee's grant, rather than to "basic research." Consequently, we recognize that it is considerably more risky, both intellectually and administratively, than our other proposals. At the same time, this proposal is considerably less expensive than the first three, and we suggest that it is important enough to be funded entirely from the present grant. Project #5 presents a plea for gathering evanescent historical and anthropological data, an idea which we believe to be a "now or never proposition." We do not propose that the Committee carry out or fund the proposal itself, but do ask that it publicize the need for gathering such data, and promote the project to government, libraries, foundations, academic institutions and individuals.

We believe that qualified personnel can be obtained to manage and staff each of these projects, though such a discussion is premature at this time. In most cases, the full costs of any of these projects can be properly estimated only after the preliminary work we recommended above (III. Conclusion: A Work Proposal) has been completed.

PROJECT #1.: Social Change in Space-Impacted Communities

We propose an interdisciplinary study of communities to determine the effects of massive technological efforts upon community culture patterns, and, within the framework, to differentiate the effects of massive space efforts from the effects of other massive technological efforts. This study would examine and compare selected "space-impacted" communities with comparable "non-space impacted" communities. Special consideration would be given to discovering such things as (a) conscious efforts to modify or adapt to impact; (b) the role of tradition or the absence of tradition in such com-

-
5. COMMENTS BY THE EXECUTIVE DIRECTOR: Projects #1, #2, and #3 as proposed involve the actual working out of research endeavors, with the consequent need for long lead-time and careful planning. In order to undertake any one of them seriously we will need additional funds. Projects #4 and #5 do not require as extensive funding. Project #4, a proposal for discussing the relation of national goals to the space program in terms of political and social philosophy and the philosophy of science, is easily manageable within the funds of our present grant. This project ought to be adopted in full, however, if we are to avoid the trap of pedestrian short-term views and emphases. The originality and uniqueness of the proposals in this project lie in the joining together of the different parts of a unified perspective. Taking each part by itself is precisely the danger we wish to avoid. Project #5, a proposal to use the resources of the Academy and the Committee for bringing about a greatly needed pair of international cooperative research projects and to promote these projects to the academic community, can also be undertaken within the resources of our present grant. --Lewis A. Dexter, 1 September 1962.

munities; (c) the degree to which professional or community status is determined by the individual's relationship to massive technological enterprises and/or to other persons engaged in the same technological enterprises; and the like. The study of social change in space-impacted communities would unquestionably have to focus on--and even might develop into--an analysis of the social indicators that one can use validly to observe changes in culture patterns.

The first step in Project #1 would be to develop criteria for the selection of communities for study. Concurrent with this first step should be a review of previous community studies and studies of professional subgroups having the characteristics of communities, with a view toward discovering in these previous works material or ideas relevant to the present problem. These efforts alone, given the present state of the social sciences and of research on the impact of technology on communities, would be valuable in and of themselves. The second step in Project #1 would be to begin preliminary field study in some of the communities selected in order to develop and test research instruments and designs. This preliminary field study would also be an excellent point at which to explore and assess practical problems of setting up community studies, e.g. - getting cooperation.

It is our conjecture that the preliminary analyses in Project #1 will reveal that the traditional geojurisdictional or ecological definitions of a community do not uncover the scientific, technological, engineering "professional communities" important to this research. Rather, the members of socio-occupational and associational groupings will be found to reside over wider geographic areas (such as Florida's Cape Canaveral area or Massachusetts' Route 128 communities) than that taken in by a community defined in traditional ecological or geojurisdictional terms. If this finding does come about--and we strongly suspect that it will--then the design and implementation of our "community" study will be more difficult than usual for such studies.⁶

PROJECT #2.: Educational Planning and Manpower Needs

The space program has helped focus new attention on education as an indispensable investment in technological progress and economic growth. Yet there has been no long-range, comprehensive educational planning in the USA to provide the context within which the space program's requirements can be considered. Nor has the space program itself been translated into specific requirements for the resources probably in shortest supply: various types of scientists, engineers, technicians and administrative personnel.

6. Additional discussion of this project can be found in an essay by John R. Seeley, "Proposed Studies of Space Program, Social Change and Community," in Appendix A to this report, available separately (mimeograph).

The objectives of Project #2 are to help fill both these gaps by developing action-oriented proposals concerning (a) the process of long-range educational planning by schools, corporations and government agencies, and (b) the manpower requirements of space programs in the USA, with special attention to their implications for educational plans.

(a) Educational planning: Education is defined here as referring to all forms of organized aids to learning. It includes, but is not limited to, the services provided by elementary and secondary schools and colleges and universities. It also includes extra-curricular activities, adult education programs, "refresher courses" for scientists and engineers, executive development, supervisory training, apprenticeship, training-on-the-job, correspondence courses and special TV and radio courses. This concept of education pays particular attention to the possible trend toward education as a life-time process of renewal and adjustment, rather than as a one-shot affair that takes place before one's entry into "real life."

Project #2 will include case studies on educational planning by different kinds of organizations--such as a large industrial corporation, a university, a school board, a diocese, a State department of education and the U.S. Office of Education. A comparative summary will be made of educational planning in Western Europe and the communist countries. This will facilitate the testing of a carefully-prepared set of hypotheses concerning planning processes in general and educational planning in particular.⁷

As a parallel line of activity in Project #2, a specific set of long-range educational goals will be postulated. These will include (i) desirable outputs and inputs of the educational system as a whole, and (ii) specific targets for each major category of educational services. These goals will be prepared from the point of view that education must be seen as a contribution to learning processes and that the result of learning processes can best be conceived of in terms of a combination of knowledge, abilities and values. The output goals will include qualitative considerations concerning the effects of education on personality and culture. The input goals will include major attention to research and development in learning processes and teaching methods. The entire set of goals will be related to probable trends and potentials in the size of the labor force, investment, consumption and the total output of the economy. Specific proposals will be developed with respect to the technical or methodological improvement of these goals. Recommendations will also be presented on how a more rational approach to goal-formulation for education can be developed within the framework of America's democratic and pluralistic society.

-
7. These hypotheses are presented in "The Administration of Public Schools" by Bertram M. Gross, Chapter 2 of The Financing of Public Education, a study prepared for the Carnegie Foundation at the Maxwell School of Syracuse University, to be published in 1963.

(b) Manpower requirements: On the manpower side, Project #2 will include a detailed analysis of various projections of manpower needs over the next two decades. This analysis will include the filling of various conceptual and methodological gaps in existing projections concerning the nature of "needs" or "requirements" and the manner in which they are estimated. It will also include special attention to the competing demands of teaching and research upon scientific personnel and to possible ways of resolving this conflict.

In addition to conclusions on manpower requirements as a whole, specific conclusions will be developed with respect to the manpower requirements of the space program. In cooperation with NASA, the Aerospace Industries Association and the appropriate labor organizations, this part of the project will yield detailed estimates of requirements for different types of scientists, engineers, technicians and administrative personnel. These estimates will be grounded on the empirical examination of manpower supply problems faced by NASA contractors and subcontractors. Special attention will be given to current and improved ways of coping with the obsolescence of knowledge and skills among space engineers and scientists as well as among others employed in space programs.

(c) Possible conferences: Plans will also be made for possible conferences on national educational goals at which the preliminary results of this project--along with relevant contributions from other participants--would be presented. Conferences of this type might lead to the crystalization of formal processes for the continuous and cooperative formulation of national educational goals in the USA and could thus be a major aid to improved educational planning by school boards, State agencies, universities, business enterprises and labor and professional organizations.

PROJECT #3.: Research Relevant to the Control of the Processes of Massive Technological Innovation

The processes of technological innovation must be clearly understood before they can be controlled to achieve our national goals. (See Project #4). We therefore propose an analysis of these processes set within a context of the broad historical development of technological innovation in the United States and other countries. Several studies that might comprise such an analysis are listed below. They can be undertaken independently, or designed as interdependent parts of a whole.

(a) An historical analysis of the processes of technological innovation: This study would focus on the evolving roles of private vs. government-sponsored R & D, as these roles are shaped by the new requirements of the innovative process. At least three major aspects of the process would be examined: (i) the sources of basic ideas and their relation to development, (ii) the nature of development and the areas of uncertainty, and (iii) the changing character of risk and uncertainty.

A number of currently accepted generalizations require thoughtful study. For example, many assume that today "R & D" necessarily involves enormous financial resources. Yet close examination of the actual processes of innovation might reveal that basic research is still relatively inexpensive, but that development has become so costly that

it often requires some kind of government support.

Another major assumption is that government-sponsored massive technological innovation (MTI) efforts are essentially programs of advanced engineering development. If this assumption is correct, then the operational success of MTI programs is virtually assured. Yet MTI programs nevertheless are run as if there were great uncertainty with respect to the pathways to success, time of completion, and ultimate cost. Consequently, such programs are government domain for private financial efforts are necessarily limited to programs that are less uncertain in these three areas. These private programs are also slower in pace and smaller in magnitude. The findings of this study would have a strong bearing on public policy with respect to future MTI undertakings. For example, the assurance of successful outcomes to seemingly risky undertakings might encourage private enterprise to initiate major civilian programs to meet national goals.

(b) The effect of MTI programs on economic growth: The overall effect of technological innovation on employment, output and economic growth is barely understood. What is needed, according to Heilbroner, is "a 'unified' theory of technology--a theory which will bring together both the impetus and the undertow of technological progress as it affects the economic process." A step towards such a theory would be a study of the effect of MTI programs on economic growth.

The importance of such a study lies in the testing of a key hypothesis: that government-sponsored MTI programs offer a fundamental policy lever by which the public might to some degree control the rate of economic growth. Economic growth is believed to depend largely on the rate of technological innovation. Important technological innovations now appear to be increasingly less dependent on market forces and more dependent on the deliberate efforts of massive programs. Many of the latter are so expensive and uncertain that they require public funding. Public funding, in turn, makes possible some public control over the rate of technological innovation and, therefore, over the rate of economic growth. We recommended that the work on this proposal be limited initially to a research design that will test the feasibility of accomplishing the larger effort within the resources available.

(c) A study of planning innovation: the advance selection of possible civilian MTI objectives: At some time in the future it might become feasible to apply the MTI process to meeting civilian needs. In that event the otherwise inevitable time lag would be eliminated if we had at hand a roster of technological projects that corresponded with the government's R & D capabilities. This calls for a study now of large-scale engineering needs that cannot be met by private enterprise alone. The focus might be on projects needed by the public sector of the economy.

(d) A program for transferring management approaches to those concerned with large-scale public undertakings: Perhaps the most valuable by-products of the government's MTI programs are the new approaches developed for solving problems on a sys-

tems basis and for planning large-scale efforts against the uncertain ties of the future.

We propose that NASA personnel work with private and government groups interested in learning to use the systems analysis approach in the solution of different physical problems of public concern. We suggest that, initially, one major urban problem be identified and worked out on a trial basis. The process of transfer would be the object of study, with special reference to applicability between settings and ease of transfer of techniques.

We also recommend that NASA study its own planning processes, generalize on them and publish the results. Again, we would like to find out how one segment of the society may learn most profitably from the experience of another. NASA should subsequently hold periodic conferences with urban planners and others who are interested in the planning problems associated with large-scale efforts and uncertainty, and the process of mutual teaching and learning should be studied and reported upon.⁸

PROJECT #4.: Space Programs and National Goals

Space programs comprise a vast series of activities intimately related to many of America's most important national goals. This is presumably the reason for the specification in NASA's grant to the Academy that the Academy's research work include "investigation of the means by which the nation's resources may be mobilized for the achievement of national goals developing from advances in science and technology, with emphasis on those goals and advances as may be particularly related to NASA objectives."

Projects #1, #2 and #3 each touch upon a different aspect of the relation between space programs and major national goals. Project #1, the proposal on space-impacted communities, deals with economic growth, democracy and individual self-development at the community level. Project #2, the proposal on educational planning, focuses on education for individual self-development and on manpower planning for economic growth. And, Project #3, the technological innovation study would deal with both economic growth and democratic institutions.

This project in contrast, concentrates upon the broad set of relations between space efforts and major national goals. It involves three major lines of inquiry:

- (a) an identification of the more important national goals related to the space program (such as peace, economic growth, democracy, national prestige, and personal self-development), with

8. Additional discussion of this project can be found in an essay by Sumner Myers, "A Study of Technological Innovation" and in testable hypotheses based on this essay by Harold Mendelsohn, both in Appendix A to this report, available separately (mimeograph).

attention in this context to the various meanings attached to each by different groups, the varying amounts of emphasis placed upon each, and the open and latent conflicts among them;

- (b) an analysis of major ways in which space programs may impinge upon each of these goal areas, with attention to whatever obstacles and conflicts may impede contributions toward goal attainment by NASA and NASA's contractors and cooperating agencies; and
- (c) an identification of major choices facing NASA and NASA's contractors and cooperating agencies with respect to contributions toward the attainment of such national goals, together with an analysis of the advantages and disadvantages of alternative courses of action.

The first stage in Project #4 will be an across-the-board "first approximation" which will attempt to deal with each of the above points in preliminary terms. This preliminary document will be prepared in close cooperation with many participants in, and observers of, the space programs. It may serve as the basis for various seminars and conferences, as well as for the preparation of the more intensive analyses.

The next stage in Project #4 could be a series of very detailed analyses of the relationship of the space program to specific national goals. The higher priority of study will be given to the goals of peace and economic growth. On each of these analyses, cooperation and assistance might be obtained from leading experts in relevant fields, from the agencies of government most concerned with these matters, and from industry and labor. However, as distinguished from Projects #1, #2 and #3, it is not undesirable to consider each part of this project as a separate matter. (Except, of course, that controversial discussion in the first stage might make it more difficult to get support for later stages; and yet unless the discussion in the first stage is controversial, its value is doubtful).

PROJECT #5.: Gathering Evanescent Historical and Anthropological Data 8b

From the standpoint of anyone interested in the study of man, one of the tragedies of the twentieth century has been the high death rate of ways of living and behaving. Rapid technological change, bringing with it the speedy dissemination and adoption of new work processes and new patterns of living, has, at the same time, meant the replacement of existing patterns of culture and technology. Insofar as the space program in-

8b. Mr. Derek de Solla Price dissents from this proposal.

volves even more systematic technological change than we have experienced before, then it will destroy existing ways of life and scrap old technology even faster than in the past.

We propose that it is important to record such evanescent historical and anthropological data for posterity. A few cultures and societies have been recorded by a brilliant and imaginative observer such as Margaret Mead. Many societies and cultures have been permitted to die without any informed observation from an anthropological and historical standpoint. Others are now dying; and, in many other instances, although the pattern of life is effectively dead, there still remain people who remember enough about it so that picture of these dead societies could be reconstructed.

Much of what we know about human behavior and the human personality has been learned from getting outside our own cultural boundaries through anthropological studies. Indeed, other cultures play a role in social science somewhat similar to that which extra-terrestrial observation of the planet may play (and from an imaginative standpoint already has played) in physics and some branches of biology. The irony of the situation is that the very technological factors which are making it possible for us to get outside this planet are destroying the varieties of culture patterns which could be studied to widen our knowledge of human behavior on this planet. Accordingly, it would be a particularly appropriate action to use funds supplied under the space program to capture, while it is still possible, some of the evanescent data about other cultures and culture patterns. Oddly enough, there has probably been more emphasis on disappearing species of birds than on disappearing patterns of culture; and while there has been worldwide cooperation on an International Geophysical Year no similar cooperation has occurred for international recording of disappearing ways of life.⁹

Actually, it would take much longer than a year (we would judge it will take five years), to organize and record in the field what we should record, for example, about those sections of Ethiopia relatively untouched by modernization; about highland groups in New Guinea before the U.S. and the Indonesians alter their ways of living; about the Caribbean Islands or the Colombian villages least touched by modern technology; and so on. There is also, of course, a need to record those patterns of life and occupations within our own country and Western Europe which may disappear because of space efforts and which are also of anthropological significance. For example, there have been few anthropological studies of gypsies.

Similar arguments concerning the need to record evanescent data also apply to the history of technology. It is our understanding that even in the field of space technology, working models of methods not actually put into operation are destroyed, forgotten or lost. This loss is probably even truer of working papers, half-developed suggestions, and the like. Yet the history of science and technology shows that such information is invaluable, both for reconstruction of what actually happened, and sometimes for re-examination of conceptions and approaches.

-
9. It may be observed that one thing which the U.S.S.R. and U.S.A. have in common is that, since 1830, each of them has destroyed many cultural variants of the greatest scientific interest, often with no trace left of the old way of living. To go a little further back, how unfortunate it is that our records about the Scotch Highlanders' unique culture patterns are so partial and biased!

(a) Recommendation #1. We recommend that the Academy undertake under its present grant from NASA the following activities:

- (i) the planning and design of an International Social Anthropology Quinquennium focusing on the issues discussed above.
- (ii) the promotion of such an idea, through cooperation with Fellows of the Academy, in relevant academic journals, associations, and media, and also through science writers and editorialists in the prestige press.

(b) Recommendation #2. We recommend that the Committee on Space Efforts and Society appoint a consultative subcommittee on the history of technology in regard in to the space program, and that this subcommittee attempt to determine how best to work with NASA in making certain that information likely to be of interest to future historians of science and technology be captured and recorded.¹⁰

B. Support for Unguided or Unanticipated Projects

We believe that it would be wise for the Committee to reserve a portion of the NASA grant for the support of smaller scale, unguided inquiries unrelated to the five projects outlined above. In this category would fall such topics as:

- 1. the social aspects of past explorations and discoveries;
- 2. the problem of anticipating confrontation with the unknown, including the possibility of superior forms of life; and
- 3. the establishment of colonies in outer space.

Several proposals and explorations have already been developed.¹¹

-
- 10. Additional discussion of this project can be found in an essay by Lewis Dexter, "Data Collection, Especially the International Social Anthropology Quinquennium," in Appendix A to this report, available separately (mimeograph).
 - 11. An interesting proposal for such limited research is contained in the essay by Harold Mendelsohn, "A Proposal for a Pilot Study of Anxieties and Anxiety-Laden Fantasies That Are Induced by Space and Space-Related Phenomena," in Appendix B to this report, available separately (mimeograph).

APPENDIX A

Detailed Statements on the Research Projects Proposed In Chapter Five

Contents

Project #1:

Seeley, John R., "Proposed Studies of Space Programs, Social Change
and Community."

Project #3:

Myers, Sumner, "A Study of Technological Innovation."

Mendelsohn, Harold, "Testable Hypotheses Derived From Myers' Essay."

Project #5:

Dexter, Lewis A., "Data-Collection, Especially the International Social
Anthropology Quinquennium."

APPENDIX B

General Essays by Members of the Summer Study Group

Contents

Gross, Bertram M., "The Applicability of the R & D Concept to the Social Sciences."

Harris, William, "Some General Observations."

Lerner, Abba P., "A Macro-Analytic Approach to the Space Effort."

Mendelsohn, Harold, "A Proposal for a Pilot Study of Anxieties and Anxiety-Laden
Fantasies that are Induced by Space and Space-Related Phenomena."

Seeley, John R., "An Alternative Focus."

For copies of any individual essay in Appendix A or B please write: The Committee
on Space Efforts and Society, 12 Garden Street, Cambridge 38, Massachusetts